

have been made. Further on the criticism reads, "I fail to see any adequate consideration of the variability of depth, of the absence of synchronism in the disturbing force in the direction of the canal." This "absence of synchronism" is precisely what the criticised equation 308 (or 311) enables us to take account of.

It seems to me that enough has been given in §§ 38, 42, 45, 55, and 63 to show that the variability in depth has not been permanently lost sight of, and also enough to convince one that "areas" as nearly uniform in depth as are many portions of the ocean can, as a first approximation, be treated as bodies having strictly uniform depths.

(2) Of course there are instances where the deflecting force due to the earth's rotation becomes important; for example, most moderately narrow arms of the sea in which the current is swift—such as the English Channel, Irish Channel, and Gulf of Georgia. But if in any of these a large stationary wave actually exists, it is hard to see how the times of its high and low waters near the loops can be seriously affected by this force, and these are the only times which chapters vi. and vii. undertake to determine. Near the nodes, when the current is swift, the deflecting force may, in a canal the width of which is but a moderately small fraction of a half wave-length, cause high water at one end of the nodal line, and at the same time low water at the other. This is true because the narrowness of the body permits its transverse slope to respond at once to the transverse forces. A progressive wave can be so superposed as to diminish or even destroy the range at one end of the nodal line while increasing the range at the other end.

Considering now a broader "area," with one or both of its lateral boundaries wanting, it is hard to see how the transverse motion occasioned by the earth's rotation can seriously interfere with the character of the stationary wave, and especially the time of elongation of the particles; for its effect cannot accumulate and so tend to produce a transverse stationary oscillation. If, on the other hand, a square or rectangular "area" about half a wave-length wide have solid lateral boundaries, it would seem that the deflecting force might, except in the equatorial regions, so alter the mode of oscillation that it could not be ignored even in the first approximation. So far as I know, there is no near approach to this case in any of the "areas" which probably exist (see Fig. 23 of my paper).

Hence, while it is true that the free oscillations in a rotating rectangular sheet of water is an unsolved problem, we see that the critic's remark, "It seems to follow that either Lord Kelvin or Mr. Harris is wrong," if in any sense true, really has very little to do with the case. In a word, taking an oscillating body as a whole, it seems to me that the oscillation, in accordance with a simple mode, can generally be regarded as the fundamental and important thing, and the effect of the earth's rotation a modifying or induced phenomenon.

(3) Now in regard to the improbability "that any large portion of our curiously shaped oceans should possess even approximately the critical free period," several things can be said. In the first place, we are not restricted to *single* half wave-lengths; the rectangular "areas" may run in any direction; the "areas" may be approximately trapezoidal, triangular, or of other forms, their free period may differ perhaps 10 per cent. or more from the period of the forces, and still have their tides greatly augmented by their approach to critical lengths. There are, indeed, portions of the ocean which cannot be covered by any areas the periods of which would be satisfactory, and in which it would be possible for the tidal forces to incite a considerable tide. Upon referring to the map, Fig. 23, it will be seen that one such region exists west of Australia, another south of New Zealand, another east of southern South America, the Arctic Ocean constitutes another. Upon referring to the map of the diurnal tides, Fig. 24, it will be seen that the South Atlantic, the South Pacific, and all of the Arctic Ocean are not regions where we can reasonably expect to find large diurnal tides.

Referring again to Fig. 23, and noting that the ocean is for the most part actually parceled out into areas of considerable width the free periods of which can hardly differ greatly from twelve lunar hours, and are, moreover, so situated that the forces do not approximately destroy one another, as can be seen by applying the rule quoted in the criticism,

it may, perhaps, be justifiable to ask how it happens that the times of high and low water at the loops, as determined by this rule, do approximately agree with observed times, unless there is some considerable truth in this "partial explanation of the tides."

Recently I have been working out in considerable detail the tides in the equatorial belt of the Indian Ocean, where it is fair to assume that the effect of the deflecting force must be small. The work goes to show that the theory set forth in the criticised paper is substantially correct. I therefore venture to refer Prof. Darwin to this discussion, which will appear in the March number of the *Monthly Weather Review*.

To avoid needless misunderstanding, it may be added here that I am well aware of the incompleteness of the treatment given in my paper. For instance, mathematicians have not up to this time been able to treat the simple problem of a rectangular "area" the rigid boundary of which consists of only two opposing end walls, although much has been done upon analogous problems relating to the open organ pipe. Even an approximate absolute value of the range of tide (excepting in small deep bodies) has not been attempted in this paper, because its determination would involve the numerical value of frictional resistance, which can be kept in abeyance when we seek only the times of tides in systems which have as free periods very nearly the tidal period. Many deductions and refinements were purposely omitted from my paper—the chief aim being simplicity. I hope eventually to be able to consider more fully matters like these in connection with detailed studies of the tides in various seas.

R. A. HARRIS.

Washington, D.C., March 28.

March Dust from the Soufriere.

SIR W. THISELTON-DYER has kindly forwarded to me a packet of volcanic dust sent to him by Dr. D. Morris, which fell in Barbados last month after an eruption of the Soufriere of St. Vincent, a brief description of which may be of interest. The sample, Dr. Morris states, was collected at Chelston, Bridgetown, on sheets laid out upon the lawn, the material being brought in and weighed every hour, and the fall continuing from 11 a.m. to 5 p.m. on the day of the eruption. It is free from all extraneous matter, and may be regarded as typical of the ash which fell on Barbados. The weight of this is estimated at about 6000 pounds (avoir.) per acre. At an average rate of three tons per acre, this would be equivalent to about 300,000 tons for the whole island.

The dust is of a dull dark brown colour, showing on close examination a minute speckling with a lighter tint. If poured on a piece of white paper and removed in the same way, a distinct warm-brown tint remains, produced by the very finest part of the powder, which is not easily removed. In Dr. Flett's excellent account of the dust which fell in Barbados after the eruption of May 7 (*Quart. Jour. Geol. Soc.*, lviii., 1902, p. 368), it is stated that this was at first brown, then slightly redder, and at last a whitish-grey impalpable powder. A bulk sample of that fall is distinctly greyer than the recent one, and a small one of the fall of 1812, in my possession, is a rather pale grey with a slight brown tinge. The new sample under the microscope differs only in detail from that described by Dr. Flett. The fragments, as a rule, do not exceed 0.01 inch, and are thus very slightly smaller than some in the May eruption; from 0.06 to 0.08 is a rather common size, and there is a fair amount of exceedingly minute dust. The principal minerals are the same, plagioclastic feldspar, hypersthene, and a green augite, but in the first steam cavities are now more abundant than glass enclosures, and I think brown glass is more often adherent, but to make certain of this point requires a fuller examination than I can give for the next few days.

T. G. BONNEY.

The Lyrid Meteors.

THE Lyrid meteors excite an interest that might be regarded as quite disproportionate to their numerical importance. They are a very rare shower, and even when considered by experienced observers as unusually abundant, they seldom appear at a higher rate than about twenty per hour.

During the past century the Lyrids have been subjected to pretty close observation. The star shower seen in America on the morning of April 20, 1803—just 100 years ago—seems to have far excelled in brilliancy its Lyrid successors, though a display witnessed, it is supposed, in 1860 in the equatorial regions of Africa is described as having rivalled in splendour the November meteor-shower of 1866. Shooting stars were seen in unusual numbers in America on April 20, 1838, and Prof. Forshey observed a Lyrid display in Louisiana on the night of April 18, 1841, when he counted sixty meteors in 2½ hours, which gives a mean rate of twenty-four per hour for one observer. On the morning of April 21, 1863, these meteors were reckoned by an English observer as appearing at the rate of forty per hour. On the night of April 18, 1876, a party of American students casually noticed that shooting stars were unusually numerous during the hours 10 to 12. Lyrid meteors were also conspicuous on the night of April 20, 1874. Mr. Denning has recorded important appearances of Lyrid meteors in 1882 and 1884, especially in the latter year on the night of April 19. The same observer has also stated that the Lyrid radiant was unusually active in 1893 and 1901, in the former on the nights of April 20 and 21, and in the latter on that of April 21. The foregoing are the most important displays on record since April 20, 1803. Periods of somewhat different lengths have been proposed with respect to the Lyrid showers, but the true period seems to be one which overlaps, and consists of nineteen years. Thus, from 1803 to 1860, we have exactly three periods of nineteen years, and from 1803 to 1841, two periods of the same length. Again, thirty-eight years, or twice nineteen years, separate the showers of 1838 and 1876. The nineteen-year period also connects the displays of 1863 and 1882, of 1874 and 1893, and of 1882 and 1901. This nineteen-year cycle is specially interesting, as it is completed at the Lyrid epoch of the present year, reckoning from the somewhat important display of April 19, 1884. A calculation made by the writer indicates that the maximum in 1903 is on April 19, 10h. 30m. G.M.T. The Lyrid radiant ought therefore to be found active in the early part of the night of April 19, probably from the hours 9 to 12. There is no prospect of Lyrids being numerous on the nights of April 20 and 21.

JOHN R. HENRY.

UNLIKE the August Perseids, the Lyrid meteor-stream, like those of the Quadrantids, Orionids and Geminids in January, October and December, seldom exhibits an abundant shooting-star display, more nearly resembling in that respect the Leonid and Bielid meteor-systems than the stream of August Perseids, its materials appearing to be still collected in one or more dense clusters in its orbit. Its brightest as well as its ordinary apparitions are also, like those of the Leonids, of remarkably short duration, so as to be very liable to escape observation unless splendid enough to arrest attention at some observing station on the globe. The great shower seen in America on the morning of April 20, 1803, only lasted in full splendour for two hours, from 1h. to 3h. a.m.; and a rather sensational abundance of the Lyrids on the morning of April 21, 1863, was entirely confined to the night of April 20, when 11 meteors, chiefly Lyrids, were seen at Hawkhurst in 45m., and 7 bright and several smaller ones were observed in 30m. at Weston-super-mare, between 11h. and 12h., and in a quarter of an hour after 15h., at Hawkhurst, 11 shooting-star tracks were noted, the meteors falling too rapidly then in all directions to be all recorded; the radiant point obtained from that night's tracks, and from a few Lyrids mapped on April 19 (23 Lyrid paths together, some of which may perhaps really have diverged from other centres), was at $277\frac{1}{2}^{\circ}$ and $34\frac{1}{2}^{\circ}$, close to the position which was first obtained of it "near α Lyrae," by Prof. E. C. Herrick, in America, 24 years earlier, on the morning of April 19, 1839. On the preceding night, of April 19, the hourly rate of meteors from 10h. to 11h. was only ordinary, and on the night of April 22, not a single meteor was seen in an hour by either of two observers who watched the clear sky simultaneously from 11h. 15m. to 12h. 15m. in London and at Hawkhurst for hoped-for accurrences.

Records of bright Lyrid showers are therefore of peculiar interest, as they may not improbably represent clusters of meteor-dust along the Lyrid stream, like some which appear

to have been noted in the stream of Leonids¹ on the mornings of November 15 and 14, in 1871 and 1872, on November 13, 1879, and on the morning of November 14, 1888, when in a watch of 2½h. until daybreak, at Bristol, Mr. Denning noted the appearance of 17 Leonids, although such strong recurrences of the shower are only rarely seen in the interval of some thirty years between the maximum Leonid displays. But the comet 1861, I., of which the Lyrid shooting-stars are supposed to be the streaming wake of pulverised materials, is one of those which it was pointed out by Prof. G. Forbes in his important paper in the *Observatory* 1888, on the probable existence of an ultra-Neptunian planet, may presumably have been captured by such a planet, and would thus be moving now with long periodic time in a very long elliptic orbit; and this would seem to be a rather serious objection to the short period of 19 years assigned in Mr. Henry's letter to the meteor, unless it should be really true, which seems hardly probable, that the meteors and the wake of dust-materials of the comet are only accidentally in extremely near agreement in their radiant points, and may yet not be actually associated together with each other in a common orbit.

In its two last returns in 1901 and 1902, the Lyrid shower was very distinctly observed to attain its greatest brightness on the night of April 21, and as this retardation of a day from its usual date of April 20 accords like the present similar retardation of the January, August, October and December showers with the postponement of all annual astronomical events by one day, since February, 1900, from the omission at the end of that month of the usual four-yearly leap-year day, attention should certainly, in the reasonable expectation of its fixity, be directed again to the night of April 21, in the approaching Lyrid period, as well as to that of April 19, which the very interestingly detailed evidence presented in Mr. John R. Henry's letter shows also to be one on which an unusually bright display of the April Lyrids may perhaps be expected.

A. S. HERSCHEL.

Observatory House, Slough, April 15.

Mendel's Principles of Heredity in Mice.

I APPRECIATE Prof. Weldon's reluctance to defend his position in a short letter, and I look forward with peculiar interest to the number of *Biometrika* where I gather this task will be undertaken.

Though deferring a reply on the simple matter of the eye-colour in the Oxford mice, Prof. Weldon finds space to ask an "explanation" of two over-lying complexities. To debate these finer points with one who doubts the Mendelian nature of the phenomena taken as a whole is like discussing the perturbations of Uranus with a philosopher who denies that the planets have orbits. Still, at the risk of diverting attention from the main issue, I will suggest how these complications may be regarded—scarcely "explained."

(1) The "lilac" mice illustrate that resolution, and partial disintegration, of characters commonly witnessed when a compound colour is crossed with an albino. The statistical value of the "lilacs" and their place in the colour-system can only be determined by further breeding. The appearance of "lilacs" or analogous types is what we expect, though their absence in the offspring of hybrids \times albinos constitutes a certain problem. This and other genuine difficulties call for careful statement and analysis.

(2) The diversity of coats in the first crosses points to heterogeneity among the gametes of one or both "pure" races. The nature of that heterogeneity is the question. Each race may breed true to colour, but the cross-bred offspring of the two is not necessarily uniform. The pigment excreted by heterozygotes may, as I could easily demonstrate, depend on factors (probably determinable) other than the visible colours of the parents, and having an independent distribution amongst their gametes. Also, while we are comprehensively assured that the coloured race was pure, the precise, if as yet uncontrolled, testimony of the records that certain individuals were *not*, seems to have

¹ From a table of principal observations of the Leonids from 1870 to 1896, in a portion of Mr. W. F. Denning's admirable review of the whole history of "The Great Meteoric Shower of November"; the *Observatory*, vol. xx. p. 201, May, 1897.